Collective Bargaining Laws and Returns to STEM Majors in the Labor Market for Teachers

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Abstract

In light of growing difficulties for schools to attract teachers in science, technology, engineering, and mathematics (STEM) fields and the continued discussions surrounding the unionization of education, this paper examines the effect of collective bargaining (CB) laws on the salary of teachers with a STEM degree. To isolate the effect of bargaining laws on STEM teachers, we compare pay differentials between STEM and non-STEM teachers in CB-mandated and -prohibited areas, controlling for the local labor market. Our results show that the bargaining laws increase female STEM teachers' salaries by approximately 7.5 percent. In exploring a potential mechanism, we demonstrate that female teachers in mandatory CB states accrue more years of experience.

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1 Introduction

School districts face difficulties finding qualified teachers in science, technology, engineering, and mathematics (STEM) fields (Feng and Sass 2018). The availability of higher-paying job opportunities outside of public education makes it challenging to recruit and retain teachers with academic backgrounds in these subjects. Indeed, the lack of adequately qualified STEM teachers has been tied to both the under-performance of US students in international math and science tests and the racial achievement gap (the National Commission on Mathematics and Science Teaching for the 21st Century 2000, Ingersoll and Perda 2010).

There is a growing recognition of the importance of improving STEM education (the National Academy of Sciences 2007), and teachers' unions are an important stakeholder in education production. It is well-established that the unions have a significant impact on education production and teacher compensation providing greater returns to experience, higher starting wages, and working conditions (Cowen and Strunk 2015). Since STEM teachers have better opportunities outside of teaching, the unions have a more substantial role in influencing their retention. For example, Han (2023) shows that weakening collective bargaining (CB) rights leads to disproportionately higher attrition among STEM teachers than non-STEM teachers. Even across gender, the unions are found to affect women differently than men, affecting the dynamics of the gender wage gap (Biasi and Sarsons 2022). Women tend to be less inclined to engage in negotiation and less likely to seek or fight for a more conducive environment for their career progress (Babcock and Laschever 2007). Taken together, these studies indicate that the teachers' unions can affect the compensation of STEM teachers by improving retention rates, which could generate differential effects across gender. In this light, we provide the first investigation into the extent to which CB rights impact the salary of STEM teachers.

In order to examine the effect of CB rights on STEM teachers' earnings, we leverage recently available information on the field of study in the American Community Survey (ACS), covering the period 2009-2018. Identifying the effect of teachers' unions on STEM teachers' pay

is challenging as unobserved heterogeneities determine both unionization across states and pay jointly. To isolate the effect of collective bargaining (CB) laws on the earnings of STEM teachers, we first use non-STEM teachers as a base comparison group and examine pay differentials between STEM and non-STEM teachers in states that mandate versus states that prohibit collective bargaining. Furthermore, our identification strategy exploits the discontinuity in CB laws at state borders and estimate models that include the local labor market fixed effects. By doing so, we utilize variation in state CB laws within the local labor market, which we define as commuting zone, controlling for possible unobservables that might be correlated with both CB laws and STEM teacher pay.

Our results suggest that CB laws have a significantly positive impact on the annual earnings of STEM teachers. Specifically, we find that STEM teachers in CB-mandated states earn approximately 5.75 percent higher earnings compared to otherwise similar teachers in states without bargaining rights. In view of heterogeneous labor markets that men and women face, women's predominance of the teaching profession, and possible differential roles of unions across genders, we examine the effects for men and women separately.[1] Our results show that the effects are concentrated among female teachers. Specifically, CB laws increase the annual earnings of female STEM teachers by nearly 7.4 percent. For male STEM teachers, the effect is imprecisely estimated.

Further supplementary analyses provide credence to the causal interpretation of our results. First, our balancing tests suggest that observable characteristics between CB and non-CB areas within commuting zones are balanced. Second, we control for various characteristics that are potentially correlated with CB laws, such as political ideology, local amenities, and economies of size, and our estimates remain almost unchanged. Finally, our results are robust to several alternative measures of union strength.

In an attempt to identify a potential mechanism behind our findings, we examine the

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[1]According to the National Center for Education Statistics, around 76.5 percent of teachers are women.

relationship between CB laws and STEM teachers' experience. We find that CB laws lead to a longer duration of experience among female STEM teachers, providing suggestive evidence that unions reduce attrition among female STEM teachers. For male teachers, the effect is imprecisely estimated, which is consistent with our baseline finding. Overall, our findings are consistent with studies in the literature (e.g., Han 2020).

In being the first paper to examine the relationship between CB rights and pay for STEM teachers, our study contributes to broadening the understanding of how union bargaining powers affect school resources. Given the critical role of unions and CB rights in the allocation of educational resources and schooling outcomes, a deeper understanding of their full impacts on labor markets for teachers is warranted to improve education policy.

The paper proceeds as follows. Section 2 previews the literature while Section 3 explains the data and Section 4 presents our empirical strategy and results. We provide robustness checks and a potential mechanism in Section 5. Section 6 concludes.

2   Previous Literature

Being influential and important parts of American public education, teachers' unions possess significant political bargaining power that allows them to influence several outcomes in the labor market for teachers. Several strands of the literature seek to understand the consequences of the unions across various dimensions, and our paper builds and expands upon three existing strands discussed below.[2]

Teacher compensation: One integral objective of teachers' unions is to bargain for higher

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[2]Moreover, our study indirectly connects to and carries implications for a relatively large literature on the role of teachers' unions on teachers' productivity, often measured by student achievement and adult earnings. The findings from these studies have been mixed. Earlier studies in this literature tend to suggest a positive effect of teachers' unions on student achievement (e.g., Eberts and Stone 1987). However, Hoxby (1996) reports that teachers' unions increase high school dropout rates by close to 2 percentage points, while Lovenheim (2009) finds no significant effects on high school dropout rates. Exploring the various provisions of collective bargaining agreements, Moe (2009), Strunk (2011), Strunk and McEachin (2011), and Marianno and Strunk (2018) conclude that more restrictive agreements generally results in a negative impact on academic achievement.

salary and other aspects of compensation, including returns to experience. Overwhelming evidence supports the notion that unions play a key role in increasing the overall salaries of teachers. Earlier studies suggest that unions and their collective bargaining powers are associated with teachers' wage premiums ranging from 3 to 20% (e.g., Baugh and Stone 1982, Freeman and Valletta 1988, Baugh and Stone 1982). More recent studies, however, find mixed results. For example, Hoxby (1996) estimates that unions raise teacher salaries by 5 percent. On the other hand, Lovenheim (2009) finds that unions lead to a negligible impact on teachers' compensation. Similarly, Frandsen (2016) uses the variation in the adoption of collective bargaining rights across the United States to estimate the effect on public employees' pay and finds little effect on teachers' pay. Freeman and Han (2012) report a 2.4 percent wage penalty for teachers in non-mandatory collective bargaining states. Leveraging a research design that considers the discontinuity in CB laws across state borders, Brunner and Ju (2019) discover that collective bargaining laws increase teachers' pay by approximately 10 percent. Beyond starting salaries, unions affect several aspects of compensation. West (2015) finds evidence of unions providing greater returns to experience and offering rewards for having a master's degree, national board certification, and seeking professional development. Existing literature analyzes all teachers as a whole, but it is plausible that the unions could affect teachers differently depending on their type. We contribute to this line of the literature by examining whether the unions affect the earnings of STEM teachers differently.

Teacher Workforce: In line with the role of teachers' unions in altering working conditions and advancing compensation schemes based on seniority and credentials rather than performance, unionization tends to affect the composition and nature of teacher workforce (e.g, Grissom and Strunk 2012). Given the literature finding on the significance of relatively smaller monetary rewards in retaining math and science teachers, an increase in salary associated with the unions can help to retain STEM teachers.[3]

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[3]For instance, Feng and Sass 2018 utilize data from Florida to provide evidence of both student loan forgiveness and one-time bonus programs significantly reducing the exit of math and science teachers. Clotfelter et al. (2008) demonstrate that an annual bonus of $1,800 reduces the turnover of math, science, and special education teachers

In addition to compensation, another channel through which unions affect teacher retention, attrition, and turnover rates is by altering the working environment. Studies commonly find that unions are associated with increased teacher retention and reduced quit rates (e.g., Johnson, Berg, and Donaldson 2005, Roth 2019). These studies support such findings by arguing that unions improve communication between teachers and school districts by providing a "voice" channel, through which teachers can express their concerns and preferences (García, Han, and Weiss 2022). Hartney and Flavin (2011) use political activity of teachers' unions as a measure of teacher union power and show that unions are associated with lowering chances of adopting reform-oriented education policies. This could make teaching tasks relatively manageable and increase the attraction of teachers to the profession. Additionally, Figlio (2002) provides suggestive evidence that unions offer non-pecuniary benefits to teachers, such as a reduction in unique course preparations.

More relevant to our study is a small literature that discusses the relationship between teacher unions and retention among STEM teachers. Ingersoll and Perda (2010) discuss that the most acute teacher shortages are in the STEM fields and attrition continues to be a major concern for schools in the United States. Using district-teacher matched data, Han (2023) finds that restricting collective bargaining increases teacher attrition, especially for male STEM teachers. Often, the argument made in this literature suggests that this is primarily due to a large wage penalty faced by current teachers which impacts the size and quality of the STEM teacher workforce (Dee and Goldhaber 2017, Hansen, Breazeale, and Blankenship 2019). Furthermore, Anzia and Moe (2014) suggest that seniority rules embedded in CB contracts facilitate experienced teachers' transfer to more favorable schools within a school district, potentially explaining why teacher unions influence teacher attrition rates by changing teachers' working environment. Our findings, which appear to be driven by an increased experience of teachers associated with collective bargaining, have implications for this strand of the literature.

Gender pay gap. Our paper is also closely related to the studies that examine the role of

by about 17 percent.

teachers' unions on the gender pay gap. Cahen (2019) uses the American Community Survey to estimate the impact of collective bargaining policies on public employee compensation. Her results indicate that collective bargaining restrictions lead to approximately 4-5% wage penalty for female public sector workers and argues that women are more likely to face structural pay discrimination when CB rights are restricted. Han (2020) focuses on public school teachers and finds that teachers unions greatly reduce the gender pay gap. The finding was pronounced in large school districts, where teacher union power would be at its highest. Using quasi-exogenous variation in the timing of the expiration of CB rights in Wisconsin, Biasi and Sarsons (2022) show that flexible pay lowered the salaries of female teachers compared to otherwise similar male teachers. The authors highlight that such an increase in pay gap is driven by female teachers engaging less frequently in negotiation over salary. Consistent with the literature, we provide evidence of CB rights affecting the salaries of STEM teachers differently across gender, thus deepening the understanding of the role of unions in the gender pay gap.

Overall, our study contributes in several ways to the ongoing debates in the literature on teachers' unions. Not only are we the first to explore the role of teachers' unions on STEM teachers' pay, but we also carefully control for any unobservable factors, education policies, and economic conditions. We do so by comparing the wage differentials between otherwise similar STEM and non-STEM teachers and by controlling for the local labor market.

3   Data

Our primary data come from the 2009-2018 American Community Survey extracted from the Integrated Public Use Microdata Series (Ruggles et al. 2019). Two features of the ACS are particularly essential for the context of our research design. First, the ACS began reporting individuals' fields of study in 2009, making it possible to identify potential STEM teachers.[4] We follow the Department of Homeland Security's STEM Designated Degree Program list to classify

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[4]It is important to note that the ACS reports the field of study that individuals majored in their bachelor's degree.

majors into the list of STEM majors.[5] In cases where there is no perfect mapping from this list to the ACS's list of college majors, we assign majors to the list of STEM majors. We present the complete list of STEM majors in Table A7. Second, the ACS is a nationwide survey based on a one percent random sample of the population. Thus, it provides a discernibly larger sample size compared to other household surveys, such as the Current Population Survey, enabling us to use the comparison of earnings in the local labor market.

Using the variable "OCC1990" that uses the 1990 Census Bureau occupational classification scheme, we define teachers as those who report being primary school teachers, secondary school teachers, and special education teachers. We exclude private school teachers as they are not subject to collective bargaining laws and have different working environments and salary schedules. We also drop teachers who classify themselves into *"kindergarten and earlier school"* and *"teachers not mentioned elsewhere"* from the data.[6] The rationale behind this is that we cannot ascertain whether they are full-time regular school teachers or represent non-school teachers such as private tutors. Additionally, we restrict the sample to ages 25-54, a group of prime-age working people, with a bachelor's degree or higher since the teaching profession requires at least a four-year college degree.[7]

The ACS provides individuals' annual earnings which is their pre-tax salary received from an employer over the past 12 months. We convert annual incomes to 2009 dollars using the Consumer Price Index (CPI) for All Urban Consumers. We drop workers whose earnings are imputed to prevent our estimates from being biased by imputation (see Bollinger and Hirsch 2006 for details regarding biases caused by earnings imputations in household surveys).

We analyze the STEM teachers' earnings in a local labor market defined as a commuting zone (CZ). A CZ comprising multiple counties is defined on the basis of journey-to-work data. The most granular level of geographical identification available in the ACS data is the Public Use

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[5]The link is https://www.ice.gov/sites/default/files/documents/Document/2016/stem-list.pdf.

[6]As we will see, we conduct a robustness check including kindergarten and earlier school teachers and our results are similar.

[7]As part of a robustness check, we expand our analysis to include the age group of 55-65 years.

Microdata Area (PUMA), which comprises a county or cluster of counties. PUMAs' delineations are based on the population data from the most recent decennial census and are updated every 10 years. Out of our sample period, 2009 to 2011 are based on the 2000 Census, and the years afterward on the 2010 Census. We use two different types of crosswalks to allocate PUMAs to CZs. We follow Autor and Dorn (2013) for the 2009-2011 period, and for the 2012-2018 period, we use Autor, Dorn, and Hanson (2019). One issue that needs to be highlighted is that some PUMAs straddle across multiple counties possibly being stratified into different CZs. This leads to a possibility of those PUMAs being mapped into more than one CZ. We allocate these PUMAs to all possible CZs. Hence, individuals from those PUMAs appear multiple times in our sample. We account for multiple reappearances by weighting by the proportional probability of each individual from each PUMA belonging to a given CZ, as in Autor, Dorn, and Hanson (2019). On top of that, we also apply the sample weight from the ACS throughout our analysis. Table 1 presents summary statistics.

Collective Bargaining Laws. We obtain information on collective bargaining (CB) statutes from Han (2019). Since federal labor laws exempt public school teachers, state statutes determine public teachers' CB provisions. And, states have introduced diverse legal systems governing CB rights in their scope and nature. Based on whether states explicitly or implicitly mandate or prohibit CB rights, we classify states into two broader categories: CB-mandated and CB-prohibited. The CB-mandated states include (ii) those that mandate CB and also allow agency fees (a total of 22 states) and (ii) those that mandate CB but ban agency fees (a total of 8 states).[8] In these mandatory states, school districts have an obligation to bargain "in good faith," when teachers' unions present demands. Acting in a bad faith would lead to stiff penalties. Likewise, CB-prohibited states include: (i) states that prohibit CB and (ii) CB "permissible" states (those that do not mandate CB but permit them). Laws in "prohibited" states explicitly ban school districts from collectively bargaining with unions. Although school districts have the

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[8]We drop Idaho, Indiana, Michigan, Tennessee, and Wisconsin as they repealed CB rights during our analysis period. However, we re-estimate our model including them in a robustness check.

freedom to engage in collective bargaining activity via meet-and-confer policies in "permissible" states, an absence of mandatory statutes has led courts to interpret such an absence as an implicit prohibition (Frandsen 2016, Brunner and Ju 2019).[9] Therefore, we place them into the classification of CB-prohibited states. Figure 1 shows a map of states that mandate CB for teachers.

4   Empirical Strategy and Results

4.1   Empirical Strategy

The main challenge in identifying the effect of collective bargaining (CB) laws on STEM teachers' salaries is that observable and unobservable economic factors and institutional arrangements may jointly determine a state's decision to grant CB rights and STEM teachers' pay. Note that decisions on granting CB laws solely fall within states' jurisdiction. As visualized in Figure 1, most states where bargaining is not mandatory are from the South. To overcome this challenge, we compare the difference in the earnings between STEM and non-STEM teachers in states that mandate CB laws to the difference in the earnings between STEM and non-STEM teachers in states that prohibit CB laws. We base the comparison within a single labor market, as defined by a commuting zone (CZ). With CZs comprised of a cluster of counties, within which people can travel to work, they should resemble similar labor market conditions and underlying amenities. We estimate the following equation:

$$ln(earnings_{icst}) = \alpha + \tau_1 STEM_{icst} + \tau_2 STEM_{icst} * CB_s + X'_{icst}\beta + \delta_{ct} + \gamma_s + \epsilon_{icst}. \quad (1)$$

In this model, $ln(earnings_{icst})$ represents the annual earnings of public school teacher i in commuting zone c, state s, and year t. STEM is a dummy variable for potential STEM

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[9]In a robustness check, we consider alternative categorizations of treatment and control groups, as well as alternative measures of union strength.

teachers. We define potential STEM teachers as those individuals who indicated they had their undergraduate degree in STEM fields. Our approach, in spirit, is similar to Ingersoll and Perda (2010). We want to note a drawback that presumably all STEM teachers may not have completed their undergraduate degree in STEM fields. Analogously, all teachers who completed their undergraduate degrees in STEM fields may not be teaching in STEM fields. Likewise, CB_s is constructed as an indicator variable for states where CB is mandatory. Note that in this model, we do not need to include a separate indicator variable for CB_s as it is subsumed by the state fixed effects γ_s. δ_{ct} is a vector of CZ-by-year fixed effects, which eliminates confounding factors that vary over time within CZs. With the possibility of a CZ being extended across state borders, teachers within a CZ can be subject to different CB rights. Inclusion of CZ fixed effects allows us to control for any CZ-level unobservables that are potentially correlated with our key variable of interest. Likewise, we use individual controls, represented by X, to improve the precision of our parameter of interest. They include marital status, race, gender, age, education, and the number of children.[10] We cluster standard errors at the state level to allow for within-state auto-correlation of the disturbance term. Our estimates are identified from CZs that straddle state boundaries where one side of the CZ has a mandatory CB law whereas the other side in the same CZ does not. A total of 33 CZs in our analytical sample fall into this category, accounting for nearly 10 percent of the total observations. We still include CZs that do not have any within-variation in CB rights in order to increase statistical power. The identifying assumption of our model is that unobservables within a CZ that are correlated with both earnings and mandatory CB laws affect both STEM and non-STEM teachers in a similar way.

Our model is essentially a difference-in-differences (DiD) model. The first difference represents the average difference in the salary between STEM and non-STEM teachers, while the second difference measures how this gap changes when a teacher is located in a mandatory CB

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[10]Marital status is an indicator variable for whether an individual is married or not. We divide the race into four groups: non-Hispanic white, Hispanic, non-Hispanic black, and other non-Hispanic races. Education is divided into a bachelor's degree and an advanced degree.

state.

4.2 Balancing Tests

To provide initial evidence that the estimates from Equation (1) have a causal interpretation, we start by performing a series of balancing tests. Specifically, we estimate the models of the following form:

$$C_{cst} = \alpha + \tau_1 CB_s + \delta_{ct} + \epsilon_{cst}, \tag{2}$$

where C_{cst} denotes demographic characteristics of commuting zone c in state s and year t, which include the vote share of the Democratic Party in a presidential election, population density, the median household income, the unemployment rate, the labor force participation rate, and the employment-population ratio.[11] We also execute similar estimations for individual characteristics constructed from the 2009-2018 ACS. The characteristics include age, female, education (an undergraduate degree or an advanced degree), married, race (white, black, Hispanic, and other race), and the number of children. The coefficient of primary interest τ_1 represents an average difference in characteristics between areas with and without mandatory CB laws within a commuting zone. Having a statistically insignificant estimate of τ_1 can be interpreted as CB regime being uncorrelated with a within-commuting-zone characteristic.

Panel A of Table 2 presents the balancing test results for state-specific CZ characteristics, while Panel B reports balancing test results for observable individual characteristics from the ACS. Note that the estimates reported in Column 1 are derived without controlling for the local labor market. The results suggest that there exist significant differences in the characteristics of CZs located in CB-mandatory and non-mandatory states. For example, CZs located in CB-mandatory states have a significantly higher median household income, a higher labor force

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[11]We obtain data on the vote share from MIT Election Data & Science Lab (https://electionlab.mit.edu/data). We collect data on the median household income from the Census Bureau (https://www.census.gov/topics/income-poverty/income/data/tables.2009.html) and county-level labor force statistics from the Bureau of Labor Statistics (https://www.bls.gov/lau/#cntyaa). We aggregate them to the commuting zone level by weighting by county population.

participation rate, and contain types of voters who are more likely to support the Democratic presidential candidate. This finding is not surprising given the self-selection of states into mandating bargaining laws as shown in Figure 1. Column 2 of Table 2 presents the results based on the comparison of the characteristics between CB-mandated and CB-prohibited areas within CZs. Out of 16 variables within a CZ comparison, coefficients on 13 of them are statistically insignificant. Only three variables (age, white, and the number of children) remain statistically significant. Overall, the balancing test results reassure that CZ characteristics are generally balanced across mandatory and non-mandatory areas, giving credibility to our research design.

4.3   Main Results

In this section, we present the results of regressions, based on Equation (1), estimating the impact of CB laws on the teachers' earnings. All models are weighted using the person weight provided by the ACS and the probability of an individual belonging to a particular CZ as defined above. Furthermore, all the specifications reported in Table 3 and subsequent tables include a full set of individual characteristics, the state fixed effects, and the commuting zone-by-year fixed effects.

Columns 1 and 2 of Table 3 report the results for the full sample. The direction of the estimated coefficient on STEM is negative, and its magnitude is $100 \times (e^{-0.071} - 1) = -6.85\%$.[12] The coefficient on STEM reflects the pay gap between STEM versus non-STEM teachers in situations where the value of CB is zero, that is, the absence of CB laws. The lower salary earned by STEM teachers in the absence of CB rights could be due to various factors, including their lower duration of teaching experience resulting from higher attrition rates among STEM teachers. However, the positive and statistically significant coefficient on the interaction between STEM and CB suggests that the gap is significantly reduced for those covered by collective bargaining. Specifically, the coefficient on the interaction between STEM and CB laws (CB*STEM) indicates

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[12]Since the dependent variable is measured in logs, we convert the coefficient on a dummy variable in this way throughout the analysis.

that STEM teachers in mandatory states are paid about 6% higher than STEM teachers in non-mandatory states. These results are qualitatively consistent with Hoxby (1996) who finds that public school teachers in states with CB rights earn approximately 5 percent more than otherwise similar teachers in states without CB rights.

Columns 3-6 of Table 3 show the results for male and female teachers, separately. This stratification is of great interest, as both groups have different experiences and opportunities in the labor market. Furthermore, women have been predominant in the profession, with 76 percent of public school teachers being women in the school year 2017-18, according to the National Center for Education Statistics. For this reason, we analyze the effects for both groups separately throughout the remainder of our analysis. As shown in columns 3 and 4, there is clear evidence that female STEM teachers not covered by collective bargaining face a larger pay penalty than those who benefit from collective bargaining. In terms of magnitude, our results suggest that mandatory CB laws are associated with an approximately 7.47 percent wage premium for female teachers in the STEM field. Interestingly, such a premium associated with CB does not remain significant for male teachers. The estimated coefficient on the STEM*CB for men presented in Columns 5 and 6 is small and negative, and it is not statistically different from zero.[13]

Our results complement the findings in a substantial body of literature that shows the impacts of teachers' unions on various aspects, including starting salaries, returns to degree, returns to experience, and their differential effects across gender (e.g., Ballou and Podgursky 2002, Grissom and Strunk 2012, Han 2020, West and Mykerezi 2011, and Biasi and Sarsons 2022). Unlike base salary, returns to degree, and returns to experience, unions may not directly bargain for teaching subject. Therefore, our results reflect the second-order or indirect effects of unions on the salaries of STEM teachers, which can stem from multiple sources such as attrition

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[13] As noted before, we dropped five states (Idaho, Indiana, Michigan, Tennessee, and Wisconsin) which changed their CB laws in 2012. As a robustness check, we repeat our analysis, including them. We bin observations from these states before 2012 in the CB-mandated group and in 2012 or after in the CB-prohibited group. As reported in Table A1, doing that yields qualitatively similar results.

and retention.

The literature studying the effects of unions on attrition and the gender pay gap helps explain and illustrate our results. If unions were able to reduce attrition, average teacher salaries would increase when unions negotiate for greater returns to experience. Considering that STEM teachers tend to have better opportunities outside of teaching, they should be more responsive to salaries and working environment (Ingersoll and May 2012, Brunner et al. 2019, Han and Hur 2022.) In the first study to investigate the role of teacher unions on attrition by teaching subject, Han (2023) shows that CB rights affect STEM teachers' attrition rates more than non-STEM teachers. This line of research shows the possibility that reduced attrition rates may amplify returns for STEM teachers through longer accumulated experience. In the next section, we explore how CB rights affect teachers' attrition by analyzing the implied duration of experience.

Furthermore, our findings that female STEM teachers benefit the most from the right to bargain collectively are consistent with an emerging literature. For example, Cahen (2019) argues that a lack of collective bargaining legislation leads to individualized labor contracts. In such an environment, women are more likely to be subjected to wage discrimination. Our results also support the findings by Han (2020) which suggests that teachers' union power is associated with a reduced gender pay gap. Relatedly, using quasi-exogenous variation in the timing of the expiration of CB rights in Wisconsin, Biasi and Sarsons (2022) show that flexible pay disproportionately lowers the salaries of female teachers compared to otherwise similar male teachers.

5 Robustness Checks and Mechanism

5.1 Robustness Checks

Despite the research design that allowed us to uncover compelling evidence on the effect of CB laws on STEM teachers, it is still possible, though of less concern, that unobservable factors are

driving our results. Therefore, we conduct robustness checks to provide further support against the role of confounding factors or other alternative explanations.

Local amenities. Brueckner and Neumark (2014) reckon that localities having strong amenities help workers to increase their rent-extraction ability leading to a larger pay relative to public sector workers. Their empirical findings show that public sector workers in states permitting collective bargaining accrue a higher wage premium. The connection between amenities and public workers' wages operates through the channel that the higher desire of potential residents to live in high-amenity areas and their larger willingness to pay for that purpose provide better opportunities for rent-extraction. This improves the influence of unionized public sector workers through increased opportunities for campaign contributions and for organizing events. Likewise, studies regarding teachers' decisions with regards to locations suggest that teachers prefer to be located and teach in high-income areas. To address a possible concern that high amenities are likely correlated with both the enactment of CB laws and teachers' earnings, we expand our main model by controlling for the median household income interacted with CB.[14] Table 4 contains the results. Columns 1 and 2 present the results for women and men, respectively. Controlling for the median household income yields qualitatively similar results. Additionally, we use three additional amenity variables following Brueckner and Neumark (2014) , which are mild temperate, dry weather, and coastal proximity.[15] As before, we interact each variable with CB and include them as additional controls. The results are qualitatively similar (Columns 3-4 of 4).

Economies of size. Another potential issue that can compound our findings arises from economies of size associated with population density (Duncombe and Yinger 2007). Economies of size characterizes that spending per pupil decreases with an increase in the number of pupils. For example, in rural areas, a considerable portion of schooling resources may go towards transportation costs, which means a lower amount of spending available for teachers. The fixed costs

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[14]We aggregate the income data to the commuting zone level by weighting by county population.
[15]We obtain these data from Brunner and Ju (2019).

of administrators such as board of directors and of physical capital such as labs and buildings decline with a rise in students. Considering that more densely populated states are more likely to have CB laws, it is a possibility that our results may be picking up such a mechanical relationship. Therefore, we run our main model controlling for population density's interaction with CB. As reported in Columns 5 and 6 of Table 4, the estimates closely resemble the baseline estimates.

Political Leaning. Given the fact that the Democratic Party is more supportive of organized labor (Rose and Sonstelie 2010), the composition of voters can influence the collective bargaining process. Using data from Pennsylvania, Babcock and Engberg (1999) show that the return to years of experience is much larger in areas where residents have affirmative views about unions. We expand our baseline specification to include the interaction between the Democratic party's vote shares in presidential elections and CB. As shown in Columns 7 and 8 of Table 4, our results are robust to this inclusion.

Additional Checks. As noted before, we conduct two additional checks to ensure that our results are not artifact of our data construction. First, we expand the age group to include individuals aged 25 to 65 years (Table A2). Second, we incorporate kindergarten and early school teachers into our analysis (TableA3). Furthermore, Goldhaber, Lavery, and Theobald (2014) show that teacher contracts have a strong geographic relationship to one another, suggesting the existence of spatial relationships and spillovers of collective bargaining in nearby school districts. Considering this, we estimate the model without commuting zone fixed effects and compare the outcomes in CB-mandated versus CB-prohibited states. This also enables us to ensure the generalizability of our results beyond cross-bordering CZs. The resulting estimates yield the same conclusion (Table A4.)

Alternative categorizations of treatment and control groups. As noted, previously, our treatment group includes states (i) that mandate CB rights and allow agency fees and (ii) that mandate CB rights but ban agency fees. We estimate the heterogeneous effects separately

for those two types of states. As reported in Columns 1-2 of Table A5, the effects appear to be concentrated among states that mandate CB rights and allow agency fees. The direction of the effect on states that mandate CB rights but ban agency fees is still positive, but it is imprecisely estimated (Columns 3-4 of Table A5). Likewise, in yet another robustness check, we only include "prohibited" states that explicitly ban school districts from collectively bargaining with unions in our control group, thus excluding "permissible" states that do not mandate CB but permit them. Columns 5-6 of Table A5 report the results, which are similar to our baselines estimates.

Using Non-Teachers as a Control Group. To further check the robustness of our findings, we use non-teachers as an additional control group. Using a new source of variation coming from the comparison between STEM teachers and non-teachers in the private sector helps to account for any unobservable confounders that affect both teachers and non-teachers within a CZ in a similar fashion. We use the following regression:

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\begin{aligned}
ln(earnings_{icst}) \quad = \quad & \alpha + \tau_1 T_{icst} + \tau_2 STEM_{icst} + \tau_3 T_{icst} * STEM_{icst} + \tau_4 T_{icst} * CB_s + \tau_5 STEM_{icst} * CB_s \\
& + \tau_6 T_{icst} * STEM_{icst} * CB_s + X'_{icst}\beta + \delta_{ct} + \gamma_s + \epsilon_{icst}.
\end{aligned}
\tag{3}
$$

In this model, T is an indicator variable that takes the value of one for public-school teachers, zero otherwise. Other terms are defined as above. τ_6, the coefficient on the interaction term $T * STEM * CB$, captures the effect of CB laws on STEM teachers. This coefficient can be interpreted as the difference in the earnings of STEM-teachers between CB-mandatory and CB-non-mandatory areas, compared to the difference in earnings between STEM-teachers and non-teachers. Table 5 presents the results for men and women separately. Female STEM teachers in mandatory states are paid about 8.3 percent higher than otherwise similar STEM teachers in non-mandatory states. This exercise further strengthens the validity of our baseline estimates.

5.2 Using Alternative Measures

In this subsection we consider alternative measures of union's strength and influence. This intends to address any concern that using an indicator measurement of CB laws may miss some variation (Lott and Kenny 2013) and to connect this study to the literature using these measures to study the impact of teacher union on various outcomes.

Union density. As a first alternative of direct measure of union power, we use the union density of teachers. In order to calculate union density across states, we use the basic monthly Current Population Survey (CPS) data from 2009 to 2018 (Flood et al. 2020). We define the density as the number of teachers who report that they are covered by unions in a state in a year by the total number of teachers in that state in that year. We standardize the union density to have a mean of zero and a standard deviation of one. We estimate the model of the following form:

$$ln(earnings_{icst}) = \alpha + \tau_1 STEM_{icst} + \tau_2 Union_{st} + \tau_3 STEM_{icst} * Union_{st} + X'_{icst}\beta + \delta_{ct} + \gamma_s + \epsilon_{icst}.$$
$$(4)$$

In this model, $Union_{st}$ represents the standardized union density in state s year t. All other variables are defined as above. Columns 1-2 of Table A6 present the results. The pattern of results is broadly similar to those with the binary measure and suggest that school unionization improves STEM teachers' earnings. Furthermore, the effect for male STEM-teachers is significant at the 10 percent level.

Union dues per teacher and union expenditures per student. Following Lott and Kenny (2013), we use union dues per teacher (total membership revenue divided by the number of full-time equivalent teachers) and union expenditure per student (total union expenditure divided by student enrollment). We standardize both variables to be mean zero and standard deviation one. Higher expenditure and dues reflect the extent of financial resources that teachers' unions mobilize towards donations to candidates running for government offices and towards lobbying for their agendas. This helps unions to be stronger. Also, applying these alternative measures

of union power also yields qualitatively similar results (Column 3-6 of Table A6).

Union index.   Finally, we use a union power index created by researchers at the Fordham Institute (Winkler and Zeehandelaar 2012) as an additional alternative measure. The index derived from bargaining status, union density, and union campaigning is expected to broadly capture the strength of union power. We also normalize the index to mean zero with a standard deviation of one. Columns 7-8 of Table A6 report the results, which are consistent with our main findings that CB rights lead to higher earnings for female STEM teachers and have a weaker effect for male STEM teachers.

5.3 Mechanism

Having established the relationship between collective bargaining (CB) laws and STEM teachers' pay, in this subsection we explore a possible mechanism behind our findings. One argument in favor of teachers' unions is that they improve the working environment for teachers, making the teaching profession more attractive. This view raises the possibility that CB power has the potential to retain STEM teachers.

STEM teachers' career choices can be viewed in the framework of neoclassical models, particularly on-the-job search, which postulate that workers strive to maximize their expected lifetime utility. In that framework, a higher wage increases the opportunity cost of outside job options net of search cost (Burdett 1978). Using panel data from Texas, Hendricks (2014) shows that higher pay to teachers increases their retention. Likewise, relative non-pecuniary benefits in teaching become an important determining factor for teacher quits (Goldhaber, Gross, and Player 2011). Using data from Wisconsin, Goldhaber, Gross, and Player (2011) provide suggestive evidence that female teachers consider their future wages when making their exit decisions. Collectively, these studies suggest that CB laws improve working conditions and future earnings prospects with the unions rewarding experience, which makes leaving a teaching career for outside opportunities more costly. Therefore, we investigate whether STEM teachers

in "mandatory" states have a longer duration of experience, which results in higher pay. To do so, we estimate Equation (1) by replacing the dependent variable with implied experience. Since the ACS data do not provide a direct measure of experience, we calculate a proxy for experience as the maximum of age minus years of schooling minus six and zero, i.e, experience=max {0, age - years of schooling - 6}.

Table 6 contains the results. We find that CB laws significantly increase female teachers' duration of experience. Female teachers in "mandatory" CB states tend to have an additional 0.77 year of experience as compared to those in "non-mandatory" CB states. We also find a positive effect for male teachers, but the magnitude is extremely small and the estimate is imprecisely estimated. In line with both the theoretical predictions and empirical findings in the literature, our results indicate that improved working conditions arising from teachers' unions could be more effective in retaining female STEM teachers. These findings are consistent with Han (2020), who provides evidence that teachers' unions reduce the teacher attrition rate.

We would like to emphasize that there could be other explanations why CB raises the STEM teachers' earnings but due to the data paucity, we cannot explore additional mechanisms here. One possibility is that CB laws help female teachers, whose negotiating power and culture over salary could otherwise be weaker, to maintain competitive salaries (Biasi and Sarsons 2022). When more data are available, future research can explore different mechanisms. Related to it, one obvious extension of our analysis is linking STEM teacher pay to student achievement. Though there is substantial literature on the link between both the relative wages of teachers and experience and schooling outcomes, it is unclear whether paying higher wages to STEM teachers translates into better student outcomes.

6   Conclusion

Over decades, teacher unionization has remained at the center of policy discussions regarding the provision of K-12 education. Due to their prominent role and scope in influencing educational

policies, emerging literature has strived to provide evidence that teachers' unions' rent-seeking behavior has enhanced various aspects of teacher compensation, such as the return to experience and base salary. However, the literature does not offer any evidence on the potential role that unions can play in influencing the earnings of STEM teachers. Understanding the role of unions in STEM teachers' pay is crucial to improving educational policies. This is especially true considering that school districts find hiring teachers in STEM fields an increasingly difficult task since STEM teachers have more lucrative non-teaching options than non-STEM teachers. Further, improving STEM education is gaining prominence in policy debates.

In this paper, we provide the first investigation of the relationship between collective bargaining laws and STEM teachers' pay. We document robust evidence that in states where collective bargaining is mandatory, female STEM teachers have around 7.47 percent higher earnings. Our baseline results on male STEM teachers are imprecisely estimated. In exploring a potential mechanism, we show that CB laws significantly increase the duration of experience of female STEM teachers. This suggests their potential role in retaining STEM teachers, a challenge that school districts have been experiencing. Future research constitutes investigating whether higher pay for STEM teachers translates into better student outcomes.

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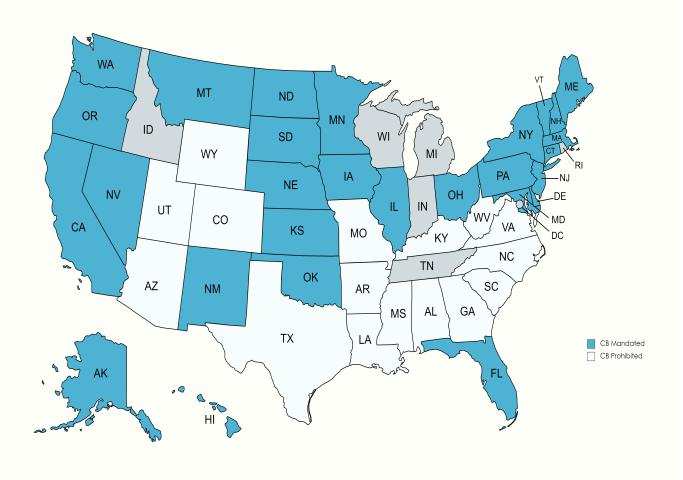
Figure 1: Collective Bargaining Laws across States

Notes: The figure visualizes collective bargaining (CB) laws across states. Darker shading indicates states where CB is mandatory. See the text for details.

Table 1: **Summary Statistics**

	Women				Men			
	CB		No CB		CB		No CB	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Log Annual Earnings	10.674	0.877	10.587	0.855	11.086	0.861	11.041	0.832
Teacher	0.126	0.332	0.183	0.386	0.046	0.210	0.055	0.227
STEM	0.167	0.373	0.160	0.367	0.370	0.483	0.366	0.482
Bachelor's Degree	0.637	0.481	0.659	0.474	0.653	0.476	0.678	0.467
Advanced Degree	0.363	0.481	0.341	0.474	0.347	0.476	0.322	0.467
Age	38.309	8.610	38.325	8.527	38.938	8.520	39.099	8.446
Married	0.606	0.489	0.626	0.484	0.660	0.474	0.699	0.459
White	0.722	0.448	0.719	0.449	0.724	0.447	0.739	0.439
Hispanic	0.079	0.270	0.078	0.268	0.073	0.260	0.077	0.267
Black	0.062	0.241	0.121	0.326	0.046	0.210	0.079	0.270
Other Race	0.137	0.344	0.082	0.274	0.157	0.363	0.105	0.307
No. of Children	0.935	1.084	0.979	1.090	1.002	1.171	1.090	1.211
N	353,250		163,938		312,864		141,012	

Notes: We provide summary statistics using the American Community Survey (ACS) data from 2009 to 2018. The first two columns include the sample of women and the last two the sample of men. We apply the weight as described in the text to calculate these statistics.

Table 2: **Balancing Test**

Dependent Variables	**Overall Comparison**	**Within CZ Comparison**
Panel A: **CZ Level Aggregated Characteristics**		
Median Household Income	6,080.023***	-641.362
	(681.677)	(3,667.636)
Population Density	90.412	-22.495
	(70.909)	(247.360)
Democratic Presidential Candidate's Vote Share	0.047***	0.049
	(0.010)	(0.052)
Unemployment Rate	-1.142***	0.379
	(0.146)	(0.677)
Employment-to-Population Ratio	0.051***	-0.003
	(0.004)	(0.024)
Labor Force Participation Rate	4.898***	-0.101
	(0.426)	(2.388)
N	7,760	7,760
Panel B: **Individual Characteristics**		
Age	-0.207*	0.417**
	(0.108)	(0.175)
Female	-0.001	0.015
	(0.007)	(0.01)
Bachelor's Degree	-0.015*	0.003
	(0.008)	(0.013)
Advanced Degree	0.015*	-0.003
	(0.008)	(0.013)
Married	-0.036***	0.003
	(0.011)	(0.01)
White	-0.027	-0.059**
	(0.028)	(0.029)
Black	-0.035***	0.048
	(0.008)	(0.037)
Hispanic	0.004	0.000
	(0.012)	(0.004)
Other Race	0.057***	0.011
	(0.018)	(0.014)
No. of Children	-0.063**	0.049***
	(0.028)	(0.015)
N	3,038,570	3,038,570
CZ×Year FEs	N	Y

Notes: The dependent variables are a vector of CZ characteristics. Each cell presents estimates from a separate regression based on Equation (2). Column 1 presents the results estimated making an overall comparison between areas with or without mandatory CB laws. Column 2 contains the results that uses CZ-by-year FEs, which allow us to compare characteristics in CB and non-CB areas within a CZ. Standard errors are clustered at the CZ level. * denotes significance at the ten percent level, ** denotes at the five percent level, and *** denotes at the one percent level.

Table 3: **Effects on the Earnings of STEM Teachers**

| | Dep. Variable: ln(Annual Earnings) | | | | | |
| | **Full Sample** | | **Women** | | **Men** | |
	(1)	(2)	(3)	(4)	(5)	(6)
STEM*CB	0.049***	0.056***	0.070***	0.072***	0.004	0.024
	[5.022%]	[5.760%]	[7.251%]	[7.466%]	[0.401%]	[2.429%]
	(0.016)	(0.017)	(0.022)	(0.023)	(0.017)	(0.016)
STEM	-0.071***	-0.089***	-0.106***	-0.118***	-0.006	-0.036***
	[-6.854%]	[-8.515%]	[-10.058%]	[-11.130%]	[-0.598%]	[-3.536%]
	(0.012)	(0.009)	(0.015)	(0.014)	(0.016)	(0.011)
N	411,906	411,906	313,545	313,545	98,167	98,167
Indiv. controls	N	Y	N	Y	N	Y
CZ×Year FEs	Y	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y	Y

Notes: The dependent variable is the log of annual earnings. We use Equation (1) to calculate estimates. Given that the dependent variable is measured in logs, we convert the coefficients to percentages using the formula, $100 \times (e^{\beta_j} - 1)$. The converted coefficients are then reported in brackets. Standard errors are clustered at the state level. * denotes significance at the ten percent level, ** denotes at the five percent level, and *** denotes at the one percent level.

Table 4: **Controlling for Local Amenities**

	Dep. Variable: ln(Annual Earnings)							
	Women	Men	Women	Men	Women	Men	Women	Men
STEM*CB	0.0718***	0.0242	0.0707***	0.0258	0.0718***	0.0242	0.0718***	0.0242
	(0.0227)	(0.0159)	(0.0231)	(0.0158)	(0.0227)	(0.0159)	(0.0227)	(0.0159)
STEM	-0.1183***	-0.0361***	-0.1181***	-0.0362***	-0.1183***	-0.0361***	-0.1184***	-0.0361***
	(0.0136)	(0.0112)	(0.0136)	(0.0111)	(0.0136)	(0.0112)	(0.0136)	(0.0112)
N	311,209	97,040	309,725	96,465	311,209	97,040	311,209	97,040
Household Income	Y							
Local Amenities			Y					
Population Density					Y			
Political Leaning							Y	

Notes: The dependent variable is the log of annual earnings. We use Equation (1) to calculate estimates. Columns 1 and 2 present the results controlling for household income, Columns 3 and 4 for mild temperate, dry weather, and for coastal proximity, Columns 5 and 6 for population density, and Columns 7 and 8 for political ideology. Standard errors are clustered at the state level. * denotes significance at the ten percent level, ** denotes at the five percent level, and *** denotes at the one percent level.

Table 5: **Using Non-Teachers as the Control Group**

	Dep. Variable: ln(Annual Earnings)	
	Women	Men
Teacher*CB*STEM	0.082***	0.038*
	(0.027)	(0.021)
Teacher*CB	0.034	0.023
	(0.023)	(0.028)
Teacher*STEM	-0.329***	-0.243***
	(0.018)	(0.009)
STEM*CB	-0.023*	-0.019
	(0.013)	(0.016)
Teacher	-0.105***	-0.342***
	(0.017)	(0.025)
STEM	0.213***	0.200***
	(0.008)	(0.009)
N	1,594,286	1,377,795
Indiv. controls	Y	Y
CZ×Year FEs	Y	Y
State FEs	Y	Y

Notes: The dependent variable is the log of annual earnings. We use the specification based on Equation (4). Standard errors are clustered at the state level. * denotes significance at the ten percent level, ** denotes at the five percent level, and *** denotes at the one percent level.

Table 6: **Potential Mechanism: Experience**

	Dep. Variable: Duration of Experience in Years	
	Women	Men
STEM*CB	0.762***	0.112
	(0.191)	(0.270)
STEM	-0.541***	0.077
	(0.151)	(0.167)
N	313,545	98,167
Indiv. controls	Y	Y
CZ×Year FEs	Y	Y
State FEs	Y	Y

Notes: The dependent variable is the duration of experience in years. We use the specification based on Equation (1) to calculate estimates. Standard errors are clustered at the state level. * denotes significance at the ten percent level, ** denotes at the five percent level, and *** denotes at the one percent level.

Appendix

CPS Data

In order to calculate union density across states, we use the basic monthly Current Population Survey (CPS) data spanning from 2009 to 2018. The CPS, which is a representative sample of the entire population in the U.S., serves as a primary source of labor market statistics such as employment, unemployment, and labor force participation. We categorize teachers into being covered by unions if they report that they are either "member of labor union" or "covered by union but not a member." We apply the weight using the variable "EARNWT," which represents a personal-level weight.

Table A1: **Adding 5 States That Repealed CB Laws**

	Dep. Variable: ln(Annual Earnings)	
	Women	Men
STEM*CB	0.066***	0.021
	(0.023)	(0.015)
STEM	-0.110***	-0.037***
	(0.014)	(0.010)
N	332,905	105,115
Indiv. controls	Y	Y
CZ×Year FEs	Y	Y
State FEs	Y	Y

Notes: The dependent variable is the log of annual earnings. Estimates are based on Equation (1). In this analysis, we add Idaho, Indiana, Michigan, Tennessee, and Wisconsin to our analytical sample. Standard errors are clustered at the state level. * denotes significance at the ten percent level, ** denotes at the five percent level, and *** denotes at the one percent level.

Table A2: **Expanding the Age Limit to 65 Years**

	Dep. Variable: ln(Annual Earnings)	
	Women	Men
STEM*CB	0.054**	0.033
	(0.022)	(0.023)
STEM	-0.100***	-0.043**
	(0.017)	(0.018)
N	405,074	125,793
Indiv. controls	Y	Y
CZ×Year FEs	Y	Y
State FEs	Y	Y

Notes: The dependent variable is the log of annual earnings. Estimates are based on Equation (1). In this analysis, we expand our sample to include individuals aged 25 to 65 years. Standard errors are clustered at the state level. * denotes significance at the ten percent level, ** denotes at the five percent level, and *** denotes at the one percent level.

Table A3:   **Including Kindergarten and Earlier School Teachers**

	Dep. Variable: ln(Annual Earnings)	
	Women	Men
STEM*CB	0.083***	0.025
	(0.027)	(0.016)
STEM	-0.121***	-0.036***
	(0.017)	(0.011)
N	328,548	98,542
Indiv. controls	Y	Y
CZ×Year FEs	Y	Y
State FEs	Y	Y

Notes: The dependent variable is the log of annual earnings. Estimates are based on Equation (1). In this analysis, we also include kindergarten and earlier school teachers. * denotes significance at the ten percent level, ** denotes at the five percent level, and *** denotes at the one percent level.

Table A4: **Without including CZ Fixed Effects**

	Dep. Variable: ln(Annual Earnings)	
	Women	Men
STEM*CB	0.067***	0.009
	(0.021)	(0.017)
STEM	-0.115***	-0.025*
	(0.013)	(0.013)
N	313,550	98,356
Indiv. controls	Y	Y
Year FEs	Y	Y
State FEs	Y	Y

Notes: The dependent variable is the log of annual earnings. The estimates are derived from a model similar to Equation (1) but without the inclusion of commuting zones fixed effects. Standard errors are clustered at the state level. * denotes significance at the ten percent level, ** denotes at the five percent level, and *** denotes at the one percent level.

Table A5:   **Alternative Categorizations of Treatment and Control Groups**

	Dep. Variable: ln(Annual Earnings)					
	Women (1)	Men (2)	Women (3)	Men (4)	Women (5)	Men (6)
STEM*CB	0.080***	0.026	0.026	0.013	0.077***	0.024
	(0.023)	(0.017)	(0.021)	(0.017)	(0.025)	(0.017)
STEM	-0.119***	-0.036***	-0.115***	-0.029**	-0.123***	-0.036***
	(0.014)	(0.011)	(0.014)	(0.011)	(0.017)	(0.013)
N	261,244	81,922	201,754	58,440	259,269	81,771
Indiv. controls	Y	Y	Y	Y	Y	Y
CZ×Year FEs	Y	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y	Y

Notes: The dependent variable is the log of annual earnings. Estimates are derived from Equation (1). Columns 1-2 present results by including only states that mandate CB and also allow agency fees within the treatment group, whereas Columns 3-4 present results by considering only states that mandate CB and but ban agency fees within the treatment group. The control group includes states as defined by Equation (1). Columns 5-6 present results by including "prohibited" states that explicitly ban collective bargaining within the control group. The treatment group includes all states mandating CB, including those that ban agency fees. Standard errors are clustered at the state level. * denotes significance at the ten percent level, ** denotes at the five percent level, and *** denotes at the one percent level.

Table A6: **Alternative Measures of Union Strength**

	Women	Men	Women	Men	Women	Men	Women	Men
	Dep. Variable: ln(Annual Earnings)
STEM*UnionDensity	0.0336***	0.0133*						
	(0.010)	(0.007)						
STEM*DuesPerStudent			0.0361***	0.0128*				
			(0.010)	(0.007)				
STEM*ExpendPerStudent					0.0358***	0.0132*		
					(0.008)	(0.007)		
STEM*UnionScore							0.0304***	0.0126*
							(0.010)	(0.007)
N	333,072	105,199	333,072	105,199	333,072	105,199	329,085	103,413
Indiv. controls	Y	Y	Y	Y	Y	Y	Y	Y
CZ×Year FEs	Y	Y	Y	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y	Y	Y	Y

Notes: The dependent variable is the annual earnings. We use Equation (1) to calculate estimates. Columns 1 and 2 present the results that use union density as a measure of union strength, Columns 3 and 4 union expenditure per student, Columns 5 and 6 union dues per teacher, and Columns 7 and 8 the union index. All these four measures of union strength are standardized to have a mean of zero and a standard deviation of one. See the text for detail. Standard errors are clustered at the state level. * denotes significance at the ten percent level, ** denotes at the five percent level, and *** denotes at the one percent level.

Table A7: **STEM Degree Codes in the ACS Data**

ACS Code	College Major	ACS Code	College Major
1103	Animal Sciences	2599	Miscellaneous Engineering Technologies
1104	Food Science		
1105	Plant Science and Agronomy	3600	Biology
1106	Soil Science	3601	Biochemical Sciences
1301	Environmental Science	3602	Botany
1302	Forestry	3603	Molecular Biology
1401	Architecture	3604	Ecology
2001	Communication Technologies	3605	Genetics
2100	Computer and Information Systems	3606	Microbiology
2101	Computer Programming and Data Processing	3607	Pharmacology
2102	Computer Science	3608	Physiology
2105	Information Sciences	3609	Zoology
2106	Computer Information Management and Science	3611	Neuroscience
2107	Computer Networking Telecommunications	3699	Miscellaneous Biology
2401	Aerospace Engineering	3700	Mathematics
2402	Biological Engineering	3701	Applied Mathematics
2403	Architectural Engineering	3702	Statistics and Decision Science
2404	Biomedical Engineering	3801	Military Technologies
2405	Chemical Engineering	4002	Nutrition Sciences
2406	Civil Engineering	4003	Neuroscience
2407	Computer Engineering	4005	Mathematics and Computer Science
2408	Electrical Engineering	4006	Cognitive Science and Biopsychology
2409	Engineering Mechanics, Physics, and Science	4008	Multi-disciplinary or General Science
2410	Environmental Engineering	5001	Astronomy and Astrophysics
2411	Geological and Geophysical Engineering	5002	Atmospheric Sciences and Meteorology
2412	Industrial and Manufacturing Engineering	5003	Chemistry
2413	Materials Engineering and Materials Science	5004	Geology and Earth Science
2414	Mechanical Engineering	5005	Geosciences
2415	Metallurgical Engineering	5006	Oceanography
2416	Mining and Mineral Engineering	5007	Physics
2417	Naval Architecture and Marine Engineering	5008	Materials Science
2418	Nuclear Engineering	5098	Multi-disciplinary or General Science
2419	Petroleum Engineering	5102	Nuclear, Industrial Radiology and Biology
2499	Miscellaneous Engineering	5206	Social Psychology
2500	Engineering Technologies	5701	Electrical and Mechanic Repairs and Technology
2501	Engineering and Industrial Management		
2502	Electrical Engineering Technology	6202	Actuarial Science
2503	Industrial Production Technologies	6108	Pharmacy, Pharmaceutical Sciences
2504	Mechanical Engineering Related Technology	6218	Management Information Systems and Statistics